Bandolier 54

What do we think?
What do we know?
What can we prove?

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We are often asked how items are chosen for ***Bandolier***. The answer is pretty straightforward. We have all come to recognise that systematic reviews of randomised trials present the highest quality of evidence for treatments. So each month we search MEDLINE and (increasingly) PubMed using the terms systematic review and meta-analysis. We hope that the new on-line version of the Cochrane Library will make it easier to access information from the 400 or so reviews now on the database.

This process gives rise to about 50 to 100 abstracts in each category for the previous 30 days or so, and with PubMed especially the information is bang up-to-date, noting papers published in the last month. Then it's a question of sorting the wheat from the chaff by looking at abstracts, and going on to read those papers which seem to be the most pertinent, or interesting, or which shed light on methods.

NNTs & NNHs

This month ***Bandolier*** concentrates on NNTs. Reviews have been chosen which allow the calculation of NNTs from information given in the papers. We have chosen nicotine replacement, from a terrific Cochrane review, antibiotic prophylaxis for colorectal surgery from an HTA review, NNTs for new antiepileptic drugs, some calculated NNTs from recent meta-analyses on tamoxifen, and NNHs from the Cochrane review of albumin in critically ill patients.

Bandolier had to get the information from the papers, and calculate the NNTs itself, as well as making some L'Abbé plots. Doing this certainly helped us understand what these papers were trying to say, and get a sensible handle on the information. Odds ratios just don't cut it, let alone some of the more obscure statistical outputs. And while we're on the subject, some of our statistical methodologists might like to scan some of the reviews being published to see how many of them use pukka statistical methods. We have a sneaky suspicion that some papers (none in this issue) are being published which just get it wrong!

In this issue

NICOTINE REPLACEMENT

Nicotine given as gum, patches, intranasal spray or inhaler is the most widely used method for helping people stop smoking. Does it work, and how good is it? A new Cochrane review [1] has pulled together the evidence.

Searching

Searching was exhaustive. Randomised trials were sought in which nicotine replacement was compared with placebo or no treatment, or studies with different doses of nicotine. Studies had to report cessation rate.

Outcomes

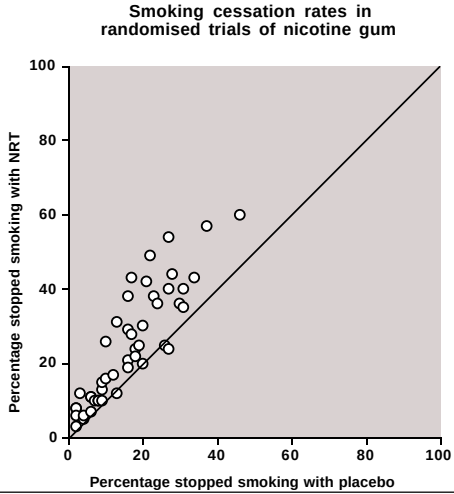
The outcome was smoking cessation, with at least six months of follow-up. The estimate at 12 months was taken when available, with six months being taken only when 12-month data were unavailable. The strictest criterion available was used to define abstinence, preference being given to biochemical confirmation of abstinence. Patients lost to follow up were regarded as being continuing smokers.

Results

The overall results for the main methods of nicotine delivery (gum, patch spray and inhaler) are shown in the Table on page 2. The overall NNT for all methods of delivery was 14 (95% CI 13 to 16). There was not much difference between different methods of delivery.

Nicotine gum

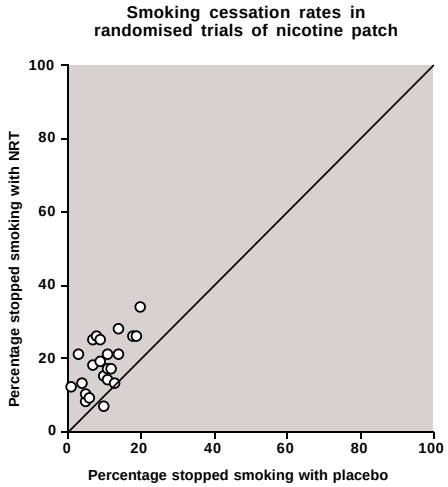
There were 47 comparisons of nicotine gum (7248 individuals) versus non-nicotine controls (9270), and in 12 of these gum was better than control. The results are shown in the L'Abbé plot. The NNT was 13 (95% CI 11 to 15).



Treatment	Number in comparison	Percent stopping with control	Percent stopping with nicotine	NNT (95%CI)
Chewing gum	16518	12	19	13 (11 to 15)
Patch	8283	9	15	16 (13 to 21)
Intranasal spray	887	12	24	8.3 (5.9 to 14)
Inhaler	976	9	17	12 (8.1 to 26)

Nicotine patch

There were 23 comparisons of nicotine patch (4511 individuals) versus non-nicotine controls (3772), and in 10 of these patch was better than control. The results are shown in the L'Abbé plot. The NNT was 16 (95% CI 13 to 21).



Intranasal spray

There were 4 comparisons of intranasal nicotine spray (448 individuals) versus non-nicotine controls (439), and in three of these spray was better than control. The results are shown in the L'Abbé plot. The NNT was 8.3 (95% CI 5.9 to 14).

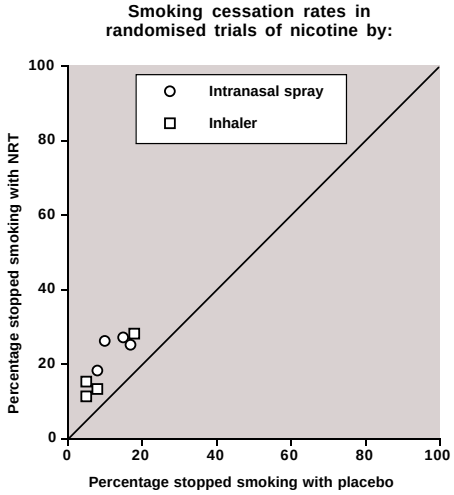
Nicotine inhaler

There were 4 comparisons of nicotine inhaler (490 individuals) versus non-nicotine controls (486), and in 2 of these inhaler was better than control. The results are shown in the L'Abbé plot. The NNT was 12 (95% CI 8.1 to 26).

Other conclusions

The report did a number of sub-group analyses to look for situations in which nicotine replacement may or may not be particularly useful. The conclusions were:

- Eight weeks of therapy is as effective as longer courses.
- Wearing patches during waking hours (16 hours) is as effective as 24 hours.
- Gum can be offered on a fixed dose or ad lib basis.
- For highly dependent smokers who fail with 2 mg gum, 4 mg gum should be offered.
- Effectiveness of nicotine therapy is independent of additional support intensity
- Nicotine therapy does not lead to increased risk of adverse cardiovascular events.



Catching them young

While we're on tobacco and smoking, two recent papers in JAMA have examined whether and how tobacco advertising can influence teenagers into becoming smokers.

The first of these [2] found a strong association in adolescents between their receptiveness to tobacco advertising and progression to experimenting with, or actually, smoking. The incidence rate of experimentation over three years to children receptive to tobacco advertising was 34%, but in those who were minimally receptive it was only 22%. The percentage of experimentation in adolescents attributable to tobacco advertising was given as 34%.

And by a quirk of fate, tobacco advertising in US magazines of those cigarette brands popular with young adolescents are much more likely to be found in magazines with high youth readership [2]. As the percentage of youth readership of magazines went up, the proportion of youth-favoured brands went up and adult-favoured brands went down. Strange that.

Reference:

1 C Silagy, D Mant, G Fowler, T Lancaster. Nicotine therapy for smoking cessation. Cochrane Library 1998 Issue 2 (date of latest amendment 27/5/98).
2 JP Pierce, WS Choi, EA Gilpin et al. Tobacco industry promotion of cigarettes and adolescent smoking. JAMA 1998 279:511-5.
3 C King, M Siegel, C Celebucki, GN Connolly. Adolescent exposure to cigarette advertising in magazines. JAMA 1998 279: 516-20.

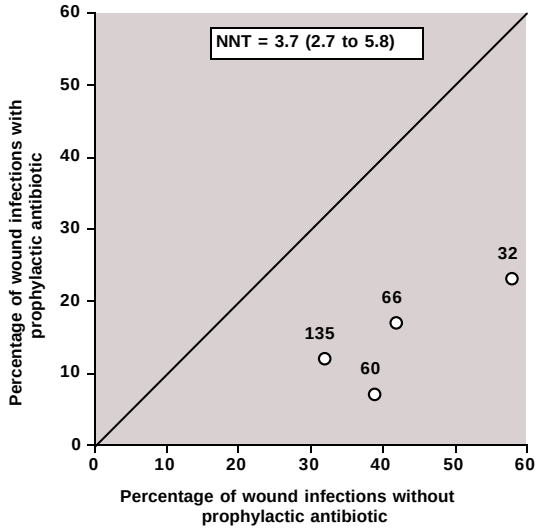
ANTIBIOTIC PROPHYLAXIS FOR COLORECTAL SURGERY

This HTA review [1] has many interesting things in it for those interested in clinical trial design. Mostly it says that serious wound infections are reduced by the use of prophylactic antibiotics (no real surprise there), but despite having found over 147 trials it was hard to say which was best.

Effectiveness

Four trials with 293 patients were comparisons of antibiotic prophylaxis with no antibiotic. Overall, 40% of patients had a serious wound infection without antibiotic compared with 13% who had a prophylactic antibiotic (Figure 1), giving a relative risk of 0.3 (0.2 to 0.5) and a NNT of 3.7 (2.7 to 5.8). None of the trials was large, but infection rates with no treatment in the trials ranged from 32 to 58%.

Figure 1: Effect of prophylactic antibiotics on wound infection in colorectal surgery (with total number in trials)



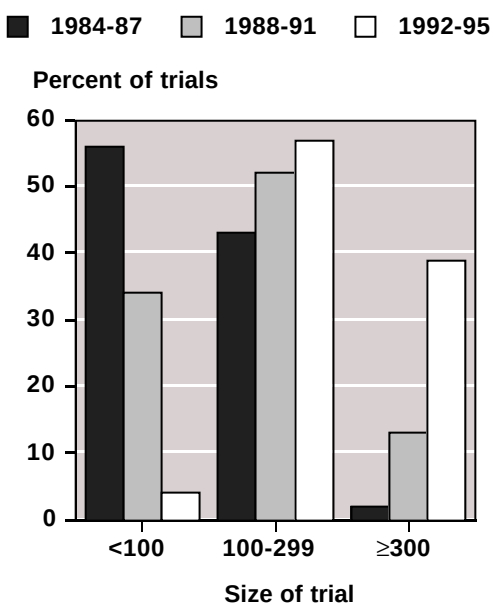
Change in patterns of trials

The authors examined the trials they found in four-year periods from 1984 to 1995. Over that period the reporting of true randomisation doubled, their definition of outcomes improved, and the proportion following up patients for at least 28 days nearly doubled. Most interesting was trial size. In the early period well over half of all trials had fewer than 100 patients, while in the later period nearly 40% of trials included over 300 patients (Figure 2).

Comment

This review is interesting and well done. What will surprise many who glance at its well-presented tables listing the trials and odds ratios won't be the odds ratios because they almost always include 1. But looking at the incidence of serious wound infections, one can find, for the same antibiotic, infection rates as low as 2% and as high as 30%. Actually most studies with prophylactic antibiotics have quite low rates of serious wound infection, so it would have been interesting to have extracted particular points from those studies with higher rates.

Figure 2: Change in trial size over the years



There is a particularly interesting discussion which pulls together important aspects of effectiveness and policy, and the authors point out that for a reasonably moderate beneficial effect of one antibiotic regimen over another, a trial would need at least 400 patients. Anyone engaged in postoperative care should take note of this review, but it also has more general interest for those who think about clinical trials and meta-analyses.

Reference:

1 F Song, AM Glenny. Antimicrobial prophylaxis in colorectal surgery: a systematic review of randomised controlled trials. Health Technology Assessment 1998 2 (7).

HTA REPORTS

Reports published by the Health Technology Assessment programmes in 1997 are:

- Antenatal screening for Down's syndrome.
- Screening for ovarian cancer: a systematic review.
- Consensus development methods, and their use in clinical guidelines development.
- A cost-utility analysis of interferon beta for multiple sclerosis.
- Effectiveness and efficiency of methods of dialysis therapy for end-stage renal disease.
- Effectiveness of hip prostheses in primary total hip replacement: critical review of evidence, and an economic model.
- Antimicrobial prophylaxis in colorectal surgery: a systematic review of randomised controlled trials.

Copies of these monographs can be ordered free of charge for NHS staff in the UK, by faxing a request saying which monograph you want with a full delivery address to 01703 595 639.

ANTIEPILEPTIC NNTS

Over the last few years several new antiepileptic (or anticonvulsant) drugs have been registered. A systematic review [1] has collected information which allowed us to calculate NNTs and NNHs in one particular clinical circumstance.

Search

A comprehensive search was used, including MEDLINE, hand searching, and use of an established database. Trials were included if they:

- recruited patients only with partial epilepsy
- they were randomised trials of add-on therapy of antiepileptics
- the treatment period was at least 8 weeks
- seizures were reported as an outcome

Outcome

The chosen efficacy outcome was the number of patients with at least 50% reduction in seizure frequency compared with baseline. Five adverse effects - ataxia, dizziness, fatigue, nausea and somnolence - were considered common and important, and information was extracted if available.

Results

NNTs for the drugs were calculated by ***Bandolier*** for any dose of drug where there were either three randomised trials or at least 100 treated patients. The NNTs are shown in Table 1 and the Figure as an NNT league table. Two of the drugs, topiramate and vigabatrin are clearly better (have lower NNTs) than tiagabine, lamotrigine, zonisamide and gabapentin (though the doses for which data were available may not be the doses commonly used). NNHs (Table 2) for the five adverse effects and treatment-related study withdrawal show no profound differences between the drugs, though with the eye of faith there is a tendency towards more effective drugs having lower (worse) NNHs.

Comment and déja vù

The authors make the point, quite rightly, that the trials they have chosen, and therefore the results, may be highly specific to the use of the antiepileptic drugs used as add-on therapy in adults with partial epilepsy. They may not be the same in people with generalised epilepsies, or in childhood epilepsies.

The authors themselves hesitate to draw differences between the effectiveness of the drugs. This may because they use odds ratios as their outcome. Odds ratios probably give the wrong answers in situations like this where the rate of events is high [2], but even if relative risks are used there is overlap of confidence intervals. The large confidence intervals for NNTs reflects the relatively small number of patients in the analyses. But most people can draw a conclusion from a simple picture like Figure 2, which is unlikely to be very wrong even if more trials are done.

Finally, some readers will have a sense of déja vù if they read the Epilepsia paper [1]. A similar review was published a year earlier by the same authors [3]. The new one contains one more trial, and has the adverse effect analysis, but, curiously, does not reference the previous paper.

References:
1 AG Marson, ZA Kadir, JL Hutton, DW Chadwick. The new antiepileptic drugs: a systematic review of their efficacy and tolerability. Epilepsia 1997 38: 859-80.
2 DL Sackett, JJ Deeks, DG Altman. Down with odds ratios! Evidence-Based Medicine 1996 Sept/Oct 1: 164-6.
3 AG Marson, ZA Kadir, DW Chadwick. New antiepileptic drugs: a systematic review of their efficacy and tolerability. BMJ 1996 313: 1169-74

Table 1: NNTs for new antiepileptic drugs in add on treatment for refractory epilepsy

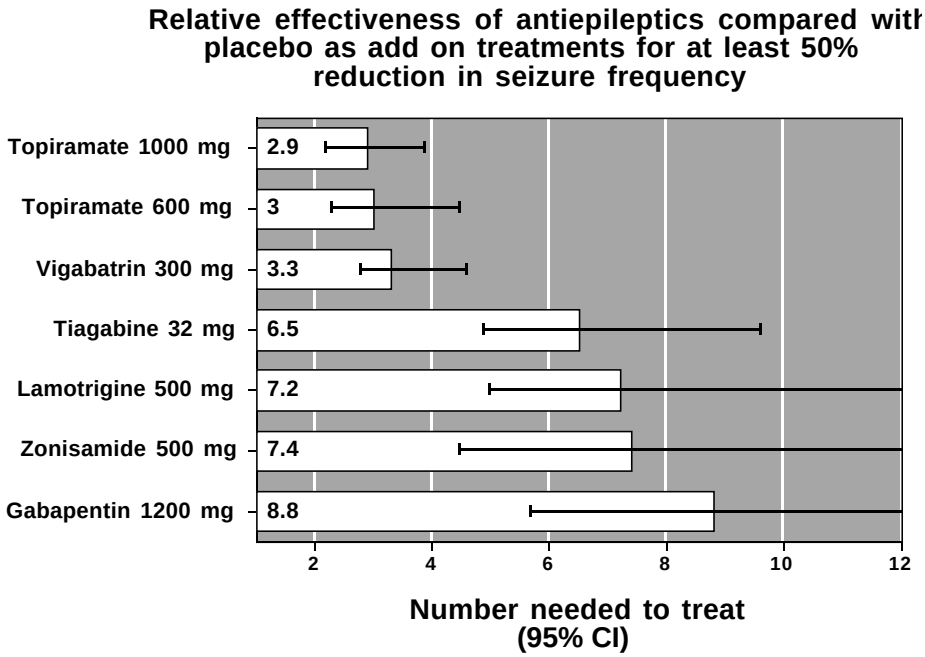
Drug	Dose (mg)	Total number of patients	NNT (95%CI)
Gabapentin	900	256	NSD
Gabapentin	1200	514	8.8 (5.7 to 20)
Zonisamide	500	291	7.4 (4.5 to 20)
Lamotrigine	500	454	7.2 (5.0 to 13)
Tiagabine	32	651	6.5 (4.9 to 9.6)
Vigabatrin	300	384	3.3 (2.6 to 4.6)
Topiramate	600	246	3.0 (2.3 to 4.5)
Topiramate	1000	303	2.9 (2.2 to 3.9)

Outcome is patients with at least 50% reduction in seizure frequency.
NSD indicates no significant difference from placebo.

Table 2: NNHs for new antiepileptic drugs in add on treatment for refractory epilepsy

	Gabapentin	Zonisamide	Lamotrigine	Tiagabine	Vigabatrin	Topiramate
Ataxia	12	5	7	NSD	NSD	12
Dizziness	9	8	5	8	14	6
Fatigue	14	8	NSD	18	10	8
Nausea	NSD	NSD	12	NSD	NSD	20
Somnolence	9	ND	22	NSD	15	5
Adverse effect withdrawal	NSD	11	25	13	12	9

NSD indicates no significant difference. ND indicates no data



EVIDENCE-INFORMED PATIENT CHOICE

There are times when what we need is answers. Other times questions are much more important. Vikki Entwistle and her colleagues from the NHS Centre for Reviews and Dissemination have written a thoughtful and provoking piece on what they call "Evidence-informed patient choice", or EIPC [1]. EIPC or evidence-based patient choice, the alternative name, trip off the tongue, but this is an area when the brain should be engaged first. Particularly so, because in the UK at least, engaging patients in decisions about their treatments has had a much increased profile in recent years. The problems are in the hows, whys, whens and how much(e)s, and even in the definition of criteria that need to be addressed.

Criteria

Entwistle and her colleagues give three criteria for EIPC, which all have to hold:

1 The decision is about which health care intervention(s) or pattern of care a person will or will not receive.
2 The person concerned is given research-based information about the effectiveness (likely outcomes, both benefits and risks) of at least two alternative interventions (which may include the option of no intervention)
3 The person concerned provides some input into the decision-making process (i.e. the decision is in some way shared between health professionals and the patient).

This definition is flexible and useful, without being proscriptive. Some might argue about the grammar, but there are always at least two options if we include doing nothing - which is an important option, as has been shown, for instance, for benign prostatic hyperplasia (***Bandolier*** 11).

Effects and thoughts

This is an impossible essay to précis. It should be read by anyone thinking about EIPC. It reviews much helpful literature, and, most of all, makes one think. And thinking before doing makes sense. Tony Hope in Oxford has also put together some useful thoughts on EIPC [2], but coming at it from a different direction, perhaps, so reading both these essays together would provide a really useful intellectual basis from which individuals or groups could proceed.

References:

1 VA Entwistle, TA Sheldon, A Sowden, IS Watt. Evidence-informed patient choice: Practical issues involving patients in decisions about health care technologies. International Journal of Technology Assessment in Health Care 1998 14: 212-25.
2 T Hope. Evidence based patient choice. King's Fund Publishing, London, 1996. ISBN 1 85717 129 2.

TAMOXIFEN TRIALS, TRIBULATIONS AND TRUTHS

Tamoxifen has been used in the treatment of breast cancer for about 25 years, but it has only been used in prevention trials, involving large numbers of women, for about 6 years, (although the first pilot study was started in 1986). The effectiveness of tamoxifen in treatment of early breast cancer and its role in prevention of breast cancer in higher risk women have recently been featured in a number of publications.

Treating early breast cancer

The Early Breast Cancer Trialists Collaborative Group led by Richard Peto and Rory Collins from the Clinical Trials Service Unit in Oxford have released an updated overview of 55 worldwide randomised trials [1] involving 37,000 women treated for early breast cancer and followed for at least 5 years, many more than 10. The overview is thought to include about 87% of the worldwide relevant evidence.

In nearly 8,000 of the women known to have a low or zero level of oestrogen receptors in their primary tumour the effects of tamoxifen treatment appeared to be small and these women were excluded from the subsequent analysis. Of the remainder, 18,000 women were known to have oestrogen receptor +ve tumours but in nearly 12,000 women receptor status was not measured (about 8,000 of these would be expected to have +ve tumours).

Benefits

In women treated with tamoxifen for 1, 2 or about 5 years, the reduction in recurrence rates during up to 10 years follow up was 21%, 29% and 47% respectively, the reduction in the incidence of contralateral breast tumours was 13%, 26% and 47% and the reduction in mortality was 12%, 17%, and 26%. The NNTs calculated from the paper for the main results for recurrence and mortality are given in Table 1. For every eight women given five years of tamoxifen treatment one would have a recurrence prevented - an NNT of 8 (95%CI 7 to 10).

Harm

The incidence of endometrial cancers was higher with tamoxifen; in absolute terms these tumours were only about half the number of contralateral breast tumours prevented. Overall, in three large trials which used tamoxifen treatment for an average of five years, the number needed to harm to produce one extra case of endometrial cancer was 97 (95%CI 68 to 168).

Looking good

The report notes that whereas in earlier studies younger (premenopausal) women seemed to benefit less than older (postmenopausal) women, the benefits found in this overview apply about equally to women of all ages. Although the report does not make specific recommendations about who should or should not be treated, (because, it argues, such decisions involve factors such as side effects and cost not reviewed in this study) nevertheless the evidence presented is strongly in favour of advising all women with oestrogen receptor positive early breast cancer to have "some" years of adjuvant tamoxifen treatment. In a separate publication Rory Collins is quoted as saying that such treatment given worldwide could save up to 20,000 lives per year. This has to be good news.

Preventing breast cancer

In stark contrast to this rather clear and relatively easily understood study stands the controversy provoked by the early disclosure of results by the US Breast Cancer Prevention Trial. This trial was started in April 1992 and closed enrolment in September 1997 with 13,388 women. The data were regularly reviewed by an independent Endpoint Review, Safety Monitoring and Advisory Committee and at its meeting in March 1998 the committee decided that in its view the benefits of tamoxifen in reducing the incidence of breast cancer had been clearly demonstrated and it recommended that the trial should be unblinded, participants and their physicians should be told which pills they had been taking, and those on placebo, and other women at increased risk, should be offered

Table 1: Tamoxifen in early breast cancer treatment

Outcome	Years of tamoxifen	NNT (95%CI)
Prevent recurrence	1	18 (13 to 30)
	2	16 (13 to 26)
	5	8 (7 to 10)
Prevent death	1	28 (18 to 66)
	2	30 (21 to 49)
	5	22 (15 to 36)
		NNH (95%CI)
Endometrial cancer	5	97 (68 to 168)

tamoxifen. This recommendation was endorsed by the National Surgical Adjuvant Breast and Bowel Project and the National Cancer Institute and the decision to close the trial was announced on 6th April.

The results on which this decision was based are shown in in Table 2. There was a 45% reduction in breast cancer, based on a total of 239 cases, but the tamoxifen treatment group had more cases of endometrial cancer, pulmonary embolism and deep vein thrombosis. There was no significant difference in heart attack rates.

The way in which the news of this trial was trumpeted around the world in the media suggested that adopting tamoxifen for prevention of breast cancer was a "no brainer". A quiet look at the numbers of events actually recorded in the trial might give one pause: there were very few. Even more pause comes from the number of adverse events recorded. People can choose themselves how to weight these outcomes, but looking at "all bad things", it is difficult to see all that much difference.

Two more trials

UK experts in this field immediately voiced their disquiet at the US decision to terminate the trial 14 months before originally planned. They felt that the follow up was too short to get a secure view of the likely long term benefits and complications. They were also concerned about the effect of the US decision on their own ongoing tamoxifen (IBIS) trial, making it more difficult to recruit women willing to be randomised to tamoxifen or placebo groups.

These concerns seem to have been vindicated by the publication of the interim results of two chemoprevention studies with tamoxifen in the Lancet of July 11th. That by Powles and colleagues at the Marsden Hospital [2] is a trial which has been running longer than any other. It was originally set up as a pilot study and only extended when regulatory problems delayed the start of the UK multicentre trial. The second report is of a trial undertaken by Veronesi and colleagues [3], largely in Italy. The UK study has recruited 2471 women and followed them for an average of nearly 6 years; the Italian study has 5408 women and an average follow up of just under 4 years.

In neither of these studies was there a statistically significant reduction in the number of breast tumours found in the tamoxifen treated women. A number of explanations for the differences have been offered but none seem convincing. Smaller studies obviously have less power to detect small differences, but both the UK and the Italian studies would have been capable of detecting a difference as great as that found in the US study. The UK study had more younger women with a stronger family history; perhaps these womens' tumours are less susceptible to prevention. They were also treated and followed up for longer than the US study and it has even been suggested that longer treatment with tamoxifen might be counter productive. The Italian study was different from the US one in that it recruited women who had had a hysterectomy, younger on average than the Americans and there was a high rate of drop-out rate, 26%.

Comment

The end point that is ultimately the most important outcome is likely to be mortality. None of these studies can yet tell us anything very useful about that. Longer follow up of the completed US trial and the ongoing ones in the UK and Europe is essential. The decision of the investigators and data monitoring committee of the IBIS trial not to unmask the results and indeed to continue to recruit patients seems a wise one.

This situation illustrates well, if slightly sadly, how difficult it is to reach universally acceptable decisions on what might at first sight might appear rather clear results. As far as the tamoxifen prevention "truth" is concerned the jury is still definitely out.

References:

1 Early Breast Cancer Trialists' Collaborative Group. Tamoxifen for early breast cancer: an overview of the randomised trials. Lancet 1998 351: 1451-67.

2 T Powles, R Eeles, S Ashley et al. Interim analyisis of the incidence of breast cancer in the Royal Marsden Hospital tamoxifen randomised chemoprevention trial. Lancet 1998 352: 98-101.

3 U Veronesi, P Maisonneuve, A Costa et al. Prevention of breast cancer with tamoxifen: preliminary findings from the Italian randomised trial among hysterectomised women. Lancet 1998 352: 93-7.

Table 2: Outcomes from US breast cancer prevention trial

Outcome	Number of events recorded in the Tamoxifen arm	Placebo arm
Invasive breast cancer	85	154
Endometrial cancer	33	14
Pulmonary embolism	17	6
Deep vein thrombosis	30	19
Fractures (hip, spine, wrist)	47	71
Deaths from breast cancer	3	5

In the trial 13,388 women were followed for a mean of 3.5 years

ALBUMIN IN CRITICAL ILLNESS

Continuing the theme of issues taken up by the media, ***Bandolier*** thought it useful to briefly summarise the systematic review on albumin in critically ill patients [1] that hit the headlines recently. As may be expected from a Cochrane review, the searching was meticulous, and great efforts were expended to find every possible trial and to obtain the numbers of deaths from authors if that was not given in the published report.

Reports and outcomes

Reports were those which had randomly administered albumin or plasma protein fraction in critically ill patients with hypovolaemia from trauma or surgery, with burns, or with hypoalbuminaemia. The outcome measure was all-cause mortality over the period of follow-up, which ranged from about 1 day until discharge.

Results

There were 32 randomised trials with 1204 patients with data for analysis (six studies had no deaths and for one no data could be obtained). Most of the trials involved fewer than 40 patients and only two involved more than 100 patients.

The overall mortality rate was 10% in controls (treated mainly with crystalloids) and 17% in those who were given albumin. Although only one trial itself had a significant result, overall the results were statistically significant for patients with hypoalbuminaemia and burns, but not (just) those with hypovolaemia. Overall the NNH for albumin treatment was 14 (95%CI 9 to 32), and the scatter of results by size of trial is shown in the L'Abbé plot.

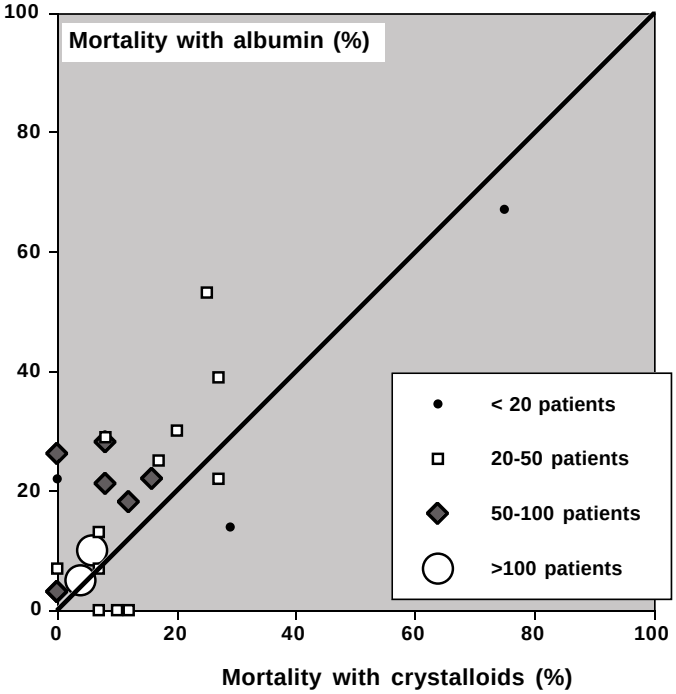
Sensitivity analysis showed that using only trials with adequate concealment of allocation did not make any difference. We have also analysed the data by trial size, and found that larger trials yielded a larger effect size. For trials with fewer than 50 patients in the comparison 17% of patients died with control and 23% with albumin, and the relative risk was 1.3 (0.9 to 1.9). For trials with at least 50 patients 6% of patients died with control and 14% with albumin, with a relative risk of 2.2 (1.4 to 3.4) and an NNH of 13 (8.5 to 27).

Comment

The authors properly acknowledge that because the review was based on small numbers of deaths the results must be interpreted with caution. They also say that a rigorous randomised trial may need to be done to confirm these results. But the size of that trial would need to be large, and the circumstances in which it could be done are not easy to discern. But this is a lovely example of systematic review uncovering a small but important effect not shown by individual studies themselves.

Reference:
1 Cochrane Injuries Group Albumin Reviewers. Human albumin administration in critically ill patients: systematic review of randomised controlled trials. BMJ 1998 317: 235-40.

Effect of abumin on mortality in all trials graded by number of patients in the comparison



Gavel

Gavel is a new journal, subtitled "Evidence-based Medicine in practice". It examines single issues, keeping a light but intelligent and intelligible touch. The first issue on lipid lowering in coronary heart disease is written by Jonathan Belsey, a former GP and HA medical advisor and published by Hayward Medical Communications. Gavel can be obtained (free of charge in the NHS) by fax on 01638 751517, or by email to admin@hayward.co.uk. Gavel is also available on the Internet and the web address is http://www.hayward.co.uk/evidence-based-medicine.

Postscript

The conference on telemedicine had to be cancelled for lack of interest, ironically on the very same day that the Prime Minister was given a demonstration of the power of the technique and spoke enthusiastically about its importance to the future of the NHS. Please let us know where we went wrong, and what we can do to entice you to our conferences. If we get sufficient encouragement we will reschedule the telemedicine conference for next year.

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